Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

The original negatives were carefully examined by the physicists present, Prof. Rowland devoting half an hour to their critical examination; and I do not think that the slightest doubt was held, by any of the six physicists present, of the completeness of the demonstration.

I cannot conceive how Dr. Pupin, after an examination of Rood's photographs, could differ from, deny, or even doubt, the conclusions reached by several of the most critical and experienced physicists of the country after their examination of these photographs.

ALFRED M. MAYER.

PSEUDO-SCIENCE IN METEOROLOGY.

IN the issue of SCIENCE for April 10th nearly a full dozen columns of valuable space have been devoted (under a rather misleading title) to recording observations and opinions which are to prove the absence of a favorable influence of forest cover on meteorological phenomena and especially on waterflow in the Western Mountains.

Since this subject has become not only one of considerable scientific interest, but also of great national importance, far-reaching economic policies depending in part on the answer which science or well sustained observation and argument can give to the question, it may not be out of place to devote further space to the question in order to warn against the many erroneous observations and fallacious conclusions contained in the article referred to.

I do not wish to offend the writer when I say that by neglecting to sift more carefully the untutored and too-often-prejudiced opinions and notions of so-called 'practical' men at the hand of the *established* facts of science, physical, physiological and meteorological, he has done harm; for he has not only increased the accumulations of 'practical' or pseudo-science, to which to be sure, many so-called 'scientists' contribute no small share, but he has also discredited the sometimes valuable—when used with discretion—observations of laymen with those men of science who read with a knowledge of the laws of physics and the facts of meteorology before them.

Sure enough meteorology, especially on the side of accurate measurements, is but poorly developed; nevertheless there is much more real knowledge in existence regarding many of the physical processes and conditions involved, not only qualitatively, but even quantitatively (as, for instance, regarding the behavior of snows, the evaporation of water, the transpiring of trees, and the conditions which influence these and the run-off and waterflow of rivers) than the writer of the article is aware of, so that it is not necessary to rely on *opinions* of 'practical' observers for these details at least.

I wish, however, not to be understood as discrediting in any way field observations and argument from them and as insisting upon accurate measurements as the only basis for the explanation of natural phenomena. On the contrary, I am one of those who believe that many complicated natural phenomena withdraw themselves for the present, *i. e.*, with our present knowledge and means, from accurate measurement; being results of complex and variable conditions which we are not prepared to measure, we may only by careful, long continued field observation and upon sound argument from well-known physical laws come to conclusions and determine relations qualitatively, leaving quantitative measure of these relations to be worked out in the future with improved method.

The present question, namely, that of forest influences on meteorological phenomena, is one of these, for in the first place we have as yet neither instruments nor methods to measure with any determinable degree of accuracy the rainfall over a given area, much less the evaporation; and even riverflow is not yet satisfactorily measured. And when it comes to the many varying influences affecting these phenomena quantitatively, we are entirely debarred from speaking with assurance even as to methods of determining them.

It would require too much space to discuss in detail the many erroneous statements and conclusions contained in the article referred to and which any meteorologist or physicist can readily discover. I shall have to confine myself to pointing out the fallacy of the main argument, which appears the more important as it has been advanced before by others with a flavor

of authority. This argument is, if I understand it correctly, that in the Western mountains the riverflow is dependent on the accumulation of winter snows ; that on the open ground these snows are drifted, accumulated and packed together, whereby the melting of the snow is retarded and the supply of available water prolonged ; that in forests the snow melts sooner, because lying less thick ; that various other causes, like mechanical obstruction to the snow in reaching the ground, transpiration, greater evaporation under trees, etc., reduce the available water supplies and hence that forests as far as waterflow is concerned are an evil. This deleterious effect, by the way, is argued almost in the same breath with which the statement is made that the forest growth in these mountains is so open, casts so little shade, accumulates so small amount of litter and offers so little obstruction to sun and wind that its effect in shading and protecting the soil and reducing evaporation may be set down as *nil*.

Now it is true that the rivers of the Sierra rely for their supply mainly on the snow waters, hence any conditions which preserve and lengthen this supply, will influence the quantity and continuity of the river flow. If, therefore, the snow drifts melt more slowly and at the same time give as much available water in proportion to the amount of snow fallen, this would be an advantage. The slow melting is true, however, only for high altitudes above timber line, which represent a comparatively small area ; below timber line the snow drifts are all gone long before midsummer, and it is only with such as lie at similar altitudes, and hence under similar temperature and wind conditions, that the condition of the snow under forest cover may be compared ; here even this seeming advantage of the snow drift, the slower melting, will be found not as great.

But the very length of time during which these snow masses are exposed to the other dissipating influences, especially the 'dry air of the mountains,' on which the writer dwells with particular emphasis, is detrimental to the amount which becomes available to the soil. It is, therefore, by no means certain whether the quantity of water delivered to the soil is in any relation to the time during which it is delivered. Knowing from tolerably reliable measurements the enormous evaporative power of air, especially when in motion, with high velocities of wind such as are common in high altitudes, we have good reason to doubt this, although undoubtedly the drifting, and hence reduction of exposed surface, reduces this loss somewhat.

It would appear much more desirable to have the snows melt quickly, provided their waters have time and opportunity to sink into the soil and away from the dissipating influences of dry air and wind, which are bound to rob the exposed drifts and leave less water for the soil. And here we reach the most important lack in the writer's argument and the most important claim of those who argue an influence of forest cover on waterflow, namely, as to the manner in which the rivers receive their water.

Even if we grant, for argument's sake, the unsubstantiated assertions of the writer, that the forest cover on these mountains is too sparse to exert any but deleterious influences with regard to conservation of snows, a contradiction in itself, he overlooks the most potent effect, which even the stumps as well as all shrubs and young growth have on the penetrability of the soil for the water.

He overlooks, as most writers on the subject do, the fact that it is not so much the surface drainage which reaches the rivers that forms the desirable supply, as the subterranean or ground waters. Surface drainage means rapid flow, high water stages, alternating with low water, uneven distribution through the year. Subdrainage means less excessive water stages, more even, steady and persistent flow, for the ground water reaches the river sometimes only *several years* after it first sank into the soil, and hence equalizes the effects of dry and wet seasons while the surface waters are carried off at once and are responsible for floods, followed by low water. Anything, therefore, that tends to change surface drainage into subdrainage is to be encouraged.

If there were, therefore, no other means by which a forest cover acted as a preserver of water supplies, the mere existence of the root system, penetrating the soil in all directions and facilitating percolation of the water, would be beneficial.

In this way, if the observation that after the removal of the old timber in Nevada the waterflow was more even be correct (which I hesitate to accept), it would find explanation in this, that the stumps and roots decayed and thereby increased the channels for the percolation of surface waters.

In conclusion I would say, that geological structure and soil conditions may be such, that percolation takes place readily even without the additional aid of a forest growth, when the effect of the latter may become irrelevant, although as a rule it may be accepted as a result of forest removal and exposure of soils, when new growth is at the same time prevented by fires and by sheep herding, that all soils become gradually more compact and less penetrable; that then more water goes over the surface and less remains for subdrainage and that ultimately the change is felt in the riverflow.

B. E. Fernow.

Washington, D. C.

ZOÖLOGY AND BIOLOGY.

To The Editor of Science: It is astonishing to find in your columns the assertion, p. 634, that the Johns Hopkins University sends out 'Doctors of Philosophy in Biology,' for you might have learned so easily that no such degree is known among us.

The examining board recommends for the degree of Doctor of Philosophy those students who have satisfactorily completed a course of study which this board has previously approved; and among all those who have been recommended for this degree during the last twenty years not a single one has presented himself for examination in biology, although many have been examined in various branches of biological science.

W. K. Brooks,

Professor of Zoölogy in the Johns Hopkins University.

Baltimore, April 28, 1896.

[The criticism of Professor Brooks is directed against a letter signed by Professor Conway MacMillan, of the University of Minnesota. Science is not responsible for the opinions of its correspondents. Ed.]

THE USE OF THE TOW-NET FOR COLLECTING PELAGIC ORGANISMS.

Editor of Science: I have so frequently seen the first use of the tow-net as a means for collecting pelagic organisms placed to the credit of Johannes Müller that I suspect many zoölogists are, as I was till recently, ignorant of the fact that Eschscholtz employed the apparatus some twenty years earlier than Müller did.

In Eschscholtz's 'Review of the Zoölogical Collection,' appended to the second volume of 'A new Voyage round the World,' by Otto von Kotzebue, I find the following on page 327: "The calms near the equator afford an abundant harvest to the zoölogist, the tranquil water presenting an immense variety of marine animals to his view, and allowing him to take them with little trouble in a net. The open woolen stuff used for flags offers the most convenient material for making these nets, as it allows the water to run through very quickly and does not stick together. A short wide bag should be made of this stuff, which may be stretched upon the hoop of a cask, and the whole fastened to a long, light pole. From the height on which we stand above the water it is impossible to perceive the smaller animals; the best way, therefore, to catch these is to hold the net half in the water, as if to skim off the bubbles of foam from the surface; then, after a few minutes, if the net is drawn out, and the interior rinsed in a glass of fresh seawater, one may frequently have the pleasure of seeing little animals of strange forms swimming in the glass. In the course of ten days I obtained, in this way, thirty-one different species of animals."

Eschscholtz does not tell us exactly when he began this kind of collecting; but the voyage on which he did it was during the years 1823, '24, '25 and 26; and as the above quotation is taken from the account of his observations in the tropical Atlantic before reaching the coast of Brazil, it certainly relates to the earlier part of the voyage.

In the last one of his series of papers on the development of Echinoderms, published in 1852, Müller tells us that he had used the tow-net 'vielen Jahren mit dem besten Erfolge.' The 'vielen Jahren,' I suppose, refers to the years during which he was prosecuting his beautiful